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BIOLOGY AS AN "EXACT" SCIENCE.

The Grammar of Science. By Karl Pearson, M.A., F.R.S., Professor of Applied Mathematics and Mechanics, University College, London. Second Edition, revised and enlarged, with 33 figures in the text. Pp. xviii + 548. (London: Adam and Charles Black, 1900.)

THE sciences of life are marked off for practical purposes from those concerned with inorganic matter by obvious differences in the nature of the material with which they respectively deal. But in addition to distinctions of this kind, it has been customary to look upon biology as having a lower claim to the title of an "exact" science than that enjoyed, for example, by chemistry and physics. This view has been emphasised by the practice of calling biology a merely "descriptive" science, with a kind of implication that other sciences are that and something more. The distinction, however, is at best an artificial one, resting mainly on the fact that the conditions of life are often so complex, and the data so difficult of access, that the use of those quantitative methods of induction which in other sciences have been fruitful of important results, so far as biology is concerned has to a great extent remained in abeyance.

It could not be expected that this state of things should be allowed to continue. "Every science," said Stanley Jevons, "and every question in science, is first a matter of fact only, then a matter of quantity, and by degrees becomes more and more precisely quantitative." In those parts of biology which come into relation with chemistry and physics, the quantitative methods have long since gained a footing. Physiology tends increasingly to become a science of exact measurement, and there is abundant scope for the exercise of mathematical power in the investigation of its present data. With regard, however, to many problems of what is known as "general biology," especially those which gather round the central doctrine of evolution, it is no doubt true that until recently measurements have either not been applied at all, or have been used only in the simplest and crudest form. That general biology has now ceased to deserve the reproach of neglecting quantitative methods is largely due to the labours of Mr. Francis Galton, Prof. Weldon, and Prof. Karl Pearson; the way towards a greater precision of method having also been in some degree prepared by other workers, such as Milne Edwards, J. A. Allen and A. R. Wallace.

In the second edition of his well-known "Grammar of Science," Prof. Pearson has included two new chapters which contain a semi-popular account of his recent work on the mathematical aspects of evolutionary theory. The ground covered is extensive, comprising quantitative investigations of variation, correlation, selection in its various forms, heredity and reversion. Those readers who may be deterred by the length and elaboration of Prof. Pearson's papers in the *Proceedings* and *Philosophical Transactions* of the Royal Society will here find a clear account of the various problems concerned, together with a tolerably easy explanation of the mathematical processes involved in their attempted solution, and a useful summary of the results so far arrived at. The author states his main position as follows:—

"What we need in the theory of evolution is quantitative measurement following upon precise definition of our fundamental conceptions. Biologists, even as physicists have done, must throw aside merely verbal descriptions, and seek in future quantitative precision for their ideas."

In the same spirit, Prof. Weldon remarked in his Presidential Address to Section D at the Bristol meeting of the British Association: "Numerical knowledge of this kind is the only ultimate test of the theory of natural selection, or of any other theory of any natural process whatever." That these dicta are substantially true will hardly be questioned, though it may be objected to Prof. Pearson that he somewhat overstates his case. All concrete science is in its essence descriptive, and it is not improbable that parts at least of biological study will have to remain indefinitely in the condition of "merely verbal description." It would appear, too, that in his eagerness to denounce the putting forward of inadequate hypotheses, the author allows himself to undervalue those rough preliminary generalisations which have frequently formed so useful a step in the completion of a great induction. It is possible to attach too much importance to Faraday's famous saying. If every "suggestive thought" which has eventually turned out to be imperfect, or even erroneous, had been "crushed in silence" instead of being given to the world, the cause of scientific progress would have suffered. We must often, for practical purposes, be content to proceed by the method of successive approximation. The work of Darwin himself was only to a limited extent quantitative.

Evolutionists of what may perhaps without offence be called the "orthodox" type, will find Prof. Pearson's attitude towards most controverted points sufficiently correct. Thus, without denying the possibility of a bathmic element in evolution, he does not countenance the "inherent growth-forces" that find favour with Neo-Lamarckians. Demonstration of the inheritance of acquired characters he holds to be still wanting; tradition, on the other hand, is probably an important factor in what are called the "instincts" of the lower animals. He finds no quantitative evidence for telegony, the occurrence of which alleged phenomenon "seems both mechanically and physiologically inconceivable." The reality of natural selection as a factor in evolution is quantitatively demonstrable, and sexual selection is rehabilitated.

It would be impossible within the limits of a notice like the present to do justice to the lucidity of Prof. Pearson's explanations, the ingenuity of his mathematical devices, and the care with which he has avoided possible sources of error in his calculations. Examples may be found in his exposition of the technical terms "modal value" and "standard deviation"; in his determination of the coefficient of regression; and in his discussion of the relative value of selective and non-selective death-rates for organs of different sizes. Among the most valuable of his suggestions are those on the importance of correlation; on selective mating in its various forms

(including autogamy, endogamy, homogamy, preferential mating or "sexual selection" in Darwin's sense, and heterogamy); and on "genetic selection" or the inheritance of fertility. The last-named principle promises to be of special weight as a factor in evolution, though the proof of definite correlation of other physical characters with that of fertility must still be considered incomplete. The analysis of natural selection into autogeneric, heterogeneric and inorganic selection ("intra-selection" being ignored) is useful, and might have been carried still further.

A contribution to the theory of evolution so original and stimulating as Prof. Pearson's must necessarily run the gauntlet of much adverse criticism. This will probably take the form rather of objection to certain points of detail than to the general drift of his method. Certainly some passages and expressions seem capable of amendment. It is, for instance, scarcely allowable to speak of the approach of the coefficient of correlation to unity as "the transition of correlation into causation." As the author himself elsewhere points out, correlation does not imply causation, though the converse is no doubt true enough. The principle of recognition-marks in their widest sense seems again to deserve more consideration than it receives at his hands. They are requisite to ensure the actual effectiveness of the impulse towards preferential mating. It is worth notice in this connection that the author's view as to the species-forming tendency of differential fertility (which is distinct from "physiological selection," as understood by Romanes) is well exemplified by Dr. Jordan's work on "mechanical selection." In speaking of hybridisation with reference to atavism, the "Grammar" does less than justice to observed facts. The evidence afforded by crosses, such as those so carefully investigated by Standfuss at Zürich and by Prof. Cossar Ewart at Penicuik, has a bearing on heredity and atavism which cannot safely be ignored. Prof. Pearson contents himself with saying that in such cases, "from physiological and mechanical reasons, the gametes produce a zygote which does not give an individual blending the ancestry. Here any singularity almost may be expected." This statement, to say the least, seems wanting in precision. Again, a severe critic might allege that the author is apt to assume theoretical values (as in the case of the resemblance of first cousins) which have not stood the test of rigid proof.

We have not yet learned to like the new term "apolegamy," nor such a phrase as "a comparative few zygotes" (p. 453). The remarkable form of a sentence on p. 461 is probably due to a printer's error, as also the substitution of DAG for FAG at the bottom of p. 447. These, however, are small matters, and do not detract from the value of the book.

We must not be led into a discussion of the earlier chapters, a notice of which appeared in these columns at the time of their original publication. There is, however, one point on which we cannot refrain from joining issue. Prof. Pearson takes biologists to task for the loose way in which they often use such terms as "matter," "force" and "motion," as if no important questions lay behind them. Now it is certain that, in their employment of these expressions, biologists have no desire whatever to prejudice any philosophical problems. When metaphysicians and physicists are agreed about the definition of these terms, the biologist will doubtless be quite ready to follow suit. Meanwhile he must be allowed the use of ordinary language. But Prof. Pearson maintains that if these words are used in their everyday, or, as he calls it, their "figurative" sense, they ought to be defined. Why so? No definition is required for the particular end in view. Supposing an opponent were to say that the "matter" of the argument was not "attractive," and that there was no "force" in this or that contention, would the Professor waste time in making him define his terms? Can we not "beat about the bush" without entering into explanations that would satisfy the schoolmaster and the botanist? It would seem that here the Professor once more overshoots his mark.

It will be convenient to give, in conclusion, a summary of the main contention of these new chapters in the author's own words, as follows:—

"It is not absence of explanations, but rather of the quantitative testing of explanations, which hinders the development of the Darwinian theory." "The problem of the near future is not whether Darwinism is a reality, but what is quantitatively the rate at which it is working and has worked."

It is noteworthy to find him adding:—

"If that problem should be answered in a way that is not in accordance with the age of the earth, as fixed by certain physicists, it by no means follows that it is biology which will have to retrace its steps. When the rate is determined, it will be as exact in its nature as physical appreciations; and it will be a question of superior logic, and not of the superiority of the 'exact' over the 'descriptive' sciences which will have to settle any disagreement of biology and physics." . . . "It is a question of the *rate* of effective change, and when the biologists are in a position to make a definite draft on the bank of time, their credit will be just as substantial as that of the so-called exact sciences."

These last sentences, as coming from a mathematician, are highly significant; and we cannot but admire the courage that has given them expression. F. A. D.

HERTZ'S MECHANICS.

The Principles of Mechanics presented in a New Form. By Heinrich Hertz. Authorised English Translation, by D. E. Jones and J. T. Walley. Pp. xxviii + 276. (London: Macmillan and Co., Ltd., 1899.)

GREAT expectations were aroused by the publication, in 1894, of a book by Heinrich Hertz, with the title, "Die Principien der Mechanik in neuem Zusammenhange dargestellt." Perhaps it would set out the received theory of dynamics in strictly logical sequence; perhaps it would present a complete theory of energy independent of the notion of force; perhaps it would bridge the gap between the molecular and mechanical standpoints. Whether it would do any of these things or not, what Hertz might have to say would certainly be worthy of attention. Hertz died before the work was printed, and the task of seeing it through the Press was entrusted to Dr. P. Lenard. He tells us that the author had devoted the last three years of his life to the book, the last two being spent in perfecting its form; and, although there are indications that he was not even then